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## DISCUSSION AND CORRESPONDENCE

## PRIORITY OVERWORKED

HAVING personally been a consistent advocate and practiser of the generally accepted rules of priority, for about fifty years,<sup>1</sup> I have no desire to criticize those who have, in recent years, taken up the subject reasonably and temperately, but it is possible in this, as in most other things, to overdo the matter. My objections to some of the recent rulings and applications of the rigid priority rule are threefold:

First: I believe that the rejection of obviously obscene names should be enforced regardless of priority. This has been done by many excellent writers.

Second: Names that have been pirated or stolen from one author by another should be rejected, if the dishonesty can be clearly shown. The cases of this kind are fortunately not numerous, but some are surprising. Such names should not be allowed to pass current any more than counterfeit money or forged checks.

Third: Names of species so badly described that they can not be identified with reasonable certainty should be rejected, especially if no type is preserved. The writings of Linné and other early writers contain many such species. The arbitrary decision of any committee does not alter the case, unless new evidence be given.

To illustrate the second proposition, I will cite a case within my personal knowledge, only omitting names and dates, for obvious reasons, although the incident is not very recent and the parties personally interested are mostly dead.

In this case two eminent and able naturalists and experts, equally interested in the same

subjects, attend the meeting of a learned society. Mr. A. reads a paper announcing the discovery of a remarkable new genus and species, say of vertebrates, giving it a MS. generic and specific name. In the description entirely new anatomical terms had to be defined. Mr. B. listens and takes notes. Within a few days B. publishes, in a scientific journal, the discovery of the identical genus and species as his own, and gives it a new name, with no reference to A. His description precedes that of A. by, say, two weeks. The former description is practically the same as the latter, only abbreviated, and even the same newly coined anatomical terms are used, thus proving that the description was a stolen one. Moreover, it afterwards develops that B. had never even seen a specimen of the creature thus described.

Under such circumstances, would the International Committee decide that the pirated descriptions and false names should be adopted in place of those of the real author?

It would be a delicate matter, perhaps, for colleagues to place before the committee requisite evidence in such a case, if recent; but if it were done, what would be the decision? Evidently under the rigid rules of priority, the names given by B. would be upheld, and later on A. would be wrongly accused of copying from B. and changing his names!

Such things have happened more than once, as many zoologists know. Again, suppose that Professor X. is monographing a large collection, say of insects, in his laboratory, to which his assistants and students have access, as is usually the case, and that one of the young men, Mr. Y., looks over his notes, lists or preliminary labels, and then publishes, without permission, the new names of genera and species in some unimportant local list of his own, without descriptions or figures, merely saying that "Professor X., in his forthcoming work, is going to describe such and such genera, with this and that species as types"; and suppose, further, that when Professor X. does publish his work he does not recognize the previous work, and uses entirely different types for the same generic names. Whose

<sup>1</sup>As an evidence of my earlier sentiments, I would call attention to the fact that in 1869 (*Am. Jour. Science*, Vol. XLVII, pp. 92-112), I reprinted the 1845 British Association "Rules of Zoological Nomenclature," with personal notes and suggestions, as footnotes, nearly all of which have been subsequently approved. See also same *Journal*, Vol. III., 1872, p. 387.

names, in such a case, should be adopted? Mr. Y. has pirated the names, but they are in print and have priority. My opinion is that they should be rejected as stolen goods.

This is not an imaginary instance, and such cases have happened more than once. Mr. Y., in such a case, may be thoughtless, rather than criminal, but the resulting confusion in nomenclature is the same.

It seems to me that the case of Fr. Weber, 1795, *versus* Fabricius, 1798, concerning the genera of Crustacea, is a case of just about this sort, yet the committee has decided in favor of the obscure and rare pamphlet of Weber, as against the important work of Fabricius, from whom the generic names were apparently stolen, or improperly borrowed, for Fabricius did not adopt or recognize many of the genera in the forms prematurely published. To adopt the pirated generic names is to throw crustacean nomenclature into much confusion.

If the unauthorized publishing of scientific names is to be upheld as valid, then a reporter for any newspaper or magazine who chooses to report technical papers and note down the names used in a meeting of a learned society may have to be quoted as the author of the names, whether rightly or wrongly spelled. I could give cases of this kind, but it is best to forget them, no doubt, for somebody might revive them, as valid publications.

To illustrate the first and third propositions we may take up an article by Professor J. Playfair McMurrich, "The Actinaria of Passamaquoddy Bay, with a Discussion of Their Synonymy."<sup>2</sup>

In this article the author tries to restore certain names given by Linné to some obscure Norwegian species, in place of those almost universally adopted by European and American writers for some of the best known species common to both coasts.

He brings forward no evidence that has not been well known to nearly all writers on the subject. He himself admits that the descriptions given by Linné are insufficient to identify any species, and he therefore depends on

the references made by Linné later (in edition XII.) to various earlier writers, as was his habit in many groups. Every one familiar with his work must recognize that he often made such references very loosely, mainly to give some general idea of the looks of a thing, without intending to imply absolute identity. McMurrich picks out certain figures, among several referred to by Linné, that he thinks can perhaps represent the species intended, but he rejects various others, and thus guesses at what Linné had in mind, even when the figures disagree with the descriptions.

In fact, Linné was profoundly ignorant in respect to most marine Invertebrates, except shells. His descriptions of Actinians are no better than an intelligent boy twelve years old could write, after five minutes of watching these creatures, and his references to figures are as careless as his descriptions. Therefore his actinian species should be dropped as indeterminable, even if there were no other good reasons. The leading European authorities, familiar with the actinians of the same region, have never been able to agree as to his species, and they surely ought to have an advantage over an American in such matters.

But this is not the only reason why most writers, before McMurrich, have wisely rejected the names. The most convincing reason has been their obscenity. No writer has been more familiar with north European actinians than P. H. Gosse. In his "Actinologia Brit.," 1860, he quotes both *A. senilis* and *A. judaica* of Linné under *A. dianthus*; and also *A. senilis* and *A. felinia* under *Tealia crassicornis*. But he dismisses these names as entirely "out of the question," on account of their objectionable significance.

Linné gave obscene names to some genera and to many species. These, in many cases, were merely the dirty names given to many marine creatures by the local fishermen and put into a Latin form by Linné or his predecessors. Such obscene names (often the same) are still in use, even by American fishermen, as I know from long experience.

<sup>2</sup> *Trans. Royal Soc. Canada*, Vol. IV., 1911, p. 59.

Once, when I asked the captain of a Cape Ann fishing schooner what names they gave to certain actinians, holothurians, ascidians, etc., he said, "We should not dare to tell our wives and daughters," and I agreed with him. Such are the names that McMurrich and some others would like to revive!

It is rather embarrassing, when asked by an educated lady the name of a beautiful sea-anemone, to have to say that its name is "*Priapus senilis*," or even *Metridium senilis*; or "*Priapus humanus*" Linné, for another creature; or to give other equally unjustifiable names.

That Linné used these and other names in an obscene sense is evident, not only because often derived from fishermen's dirty names, but because he described his species in the terms of human anatomy of sexual organs, in many cases, too absurd to mention.

It is, therefore, unfortunate that a zoologist of such excellent ability as Professor McMurrich, should waste his time trying to revive these old, dirty, indeterminable names, which he himself admits can not be definitely applied to any species by means of the descriptions themselves, while his indirect evidence is equally uncertain. The names that he thus adopts are *Metridium senilis* for *M. dianthus*; *Urticina felina* for *U. crassicornis*; *Priapus equinus* for *Actinia mesembryanthemum*.

In the tenth edition of the "Syst. Nat.," 1758, p. 656, the two species of "*Priapus*" are *P. equinus* and *P. humanus*. The latter is a sipunculoid worm. I do not know that any one has recently tried to revive this name. It has better claims than some of the others.

For *P. equinus* the only description (1758) is "semiovalis læviusculus." Surely not very edifying! In Fauna Suecica, p. 510, he has three more species: *P. senilis*; *P. judaicus*; *P. felinus*. The first has, as a diagnosis, only this: "subcylindricus rugosus," with a three-line descriptive note, to the effect that it is the size of the last joint of a finger; that it is fuscous, sordid, rough, with a subcoriaceous tunic, with the upper part soft, thin and sanguineous. These characters surely do

not apply to *M. dianthus*, which is large, soft and smooth throughout, and especially delicate and translucent, when as small as the one mentioned by Linné. It does not have the upper part sanguineous, however much it may vary in color. There are other species on the Norwegian coast that agree with the brief description far better. This identification by McMurrich is then in itself untenable, as well as undesirable.

As for "*Priapus felinus*," 1761, the case is no better. The diagnosis is "cylindricus lævis glande muricata." The descriptive note is "simillarius priori," "sed glande muricata." No reference to earlier works. What he means by a "muricate glans" is hard to understand, if he had a soft actinian before him, like *Actinia mesembryanthemum*. Perhaps he refers here to another sipunculoid worm.

As for the generic name *Priapus*, 1758, if it is to be used at all, it must be applied to the second species, *humanus*, as the type, for the first species was very early (1767) placed in *Actinia*. Whether helminthologists will adopt the name remains to be seen.

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#### THE EDUCATIONAL VALUE OF MATHEMATICS

TO THE EDITOR OF SCIENCE: In a speech before the Cincinnati Schoolmaster's Club on February 21, 1914, Professor E. L. Thorndike, of Columbia University, made certain statements with regard to the educational value of mathematics and the classical languages, which were quoted in the issue of the *Cincinnati Enquirer* for February 22. One of the statements as quoted was that

the old notion that Latin or mathematics made the mind more effective in all the work of business, law or other professions was largely superstition.

The phraseology of this statement is certainly misleading. By the use of the expression "old notion" Professor Thorndike tends to convey the impression that no up-to-date, intelligent person has such a notion. That this is the very reverse of the truth may be seen by quoting from an article by Professor C. J. Keyser in the issue of SCIENCE for